

Quantitative Estimates of Evapotranspiration in California

Jean L Woods

Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0283: Quantitative Estimates of Evapotranspiration in California

Final Panel Rating
inadequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

Authors provide no data showing that the current ET measures are not accurate or that their data would be any better. Nor is there any reference to an agency making poor decisions because these data are not as accurate as possible. Only a reference to new and different irrigation techniques that are likely to be different is used to justify their approach. It is difficult to be enthusiastic about a proposal where the first part of the project is to review methods used to measure evapotranspiration. I would expect the PIs to already have a good grasp of the methods and limitations of current and past ones before sitting down to write the proposal

Additional Comments:

Authors plan to make data available through publications and on-line. It is not clear how this would be used by the state in managing water. They also plan to participate in any new forums established by the state. Note that there is no publication track record in the proposal, although one may exist somewhere. This should be clear somewhere in the proposal given the budget request.

Authors provide no data showing that the current ET measures

Technical Synthesis Panel Review

are not accurate or that their data would be any better. Nor is there any reference to an agency making poor decisions because these data are not as accurate as possible. Only a reference to new and different irrigation techniques that are likely to be different is used to justify their approach. It is difficult to be enthusiastic about a proposal where the first part of the project is to review methods used to measure evapotranspiration. I would expect the PIs to already have a good grasp of the methods and limitations of current and past ones before sitting down to write the proposal

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

The external technical reviewers and the panel agreed that this proposal had substantial deficiencies. The proposed work, while potentially worthwhile, is not scientific research. The proposal does not state hypotheses that will be tested, nor does it demonstrate that the proposed work will improve our basic understanding and knowledge regarding the estimation of evapotranspiration. The proposed work is largely monitoring. In addition, the benefits resulting from this proposed work were not adequately justified. For example, the inadequacy of the methods currently used was not recognized, and the application of this data was not described in detail. In addition, the panel considered the review of current methods for estimating evapotranspiration to be an appropriate starting point for developing the proposal, rather than the initial stage of the project itself.

Technical Review #1

proposal title: Quantitative Estimates of Evapotranspiration in California

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	<p>The objective of this proposal was clearly stated: To update and publish (but not through peer-reviewed journals) statewide vegetative water use information for California. This is a good, solid, clear objective, and was internally consistent throughout the proposal. The proposed plan is to essentially update "Bulletin 113-4, Crop Water Use in California (1986)". The goal and ultimate product of the work is an update of this Bulletin. The justification for this, as presented by the authors, is that "Since that time major changes in irrigation practices and crop varieties have occurred, and technology available for measuring crop water use has advance." There was no mention, however, of what these major changes were, or how they might change in the future.</p> <p>Also, not a single hypothesis was presented. They could have given the previous water use maps, then developed hypotheses as to how and why it has changed, then develop a modeling exercise to show how climate, population growth, and land-use (crop shifts) change may, and in-turn, affect future water use patterns. Also, I've never seen a proposal where none of the listed references were cited in the text.</p> <p>The "idea", namely to asses the spatial and temporal variation in California's crop-based water use, is important and worthy of study. How the authors propose to accomplish this task, however, was not worthy of</p>
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#0283: Quantitative Estimates of Evapotranspiration in California

Technical Review #1

	funding.
Rating	good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	<p>No. The "tools" that will be used (Penman-Monteith-based models, Penman-based models, crop coefficients, evaporation pans, Crop efficiency parameter, etc) have been studied for decades, and I don't see how the proposed work will further our understanding of any of these at all. The justification for updating of the Bulletin is that major changes have occurred (as described above) since 1986. I believe that to be entirely true, but nowhere were the changes described, or nowhere was it hypothesized how these changes are impacting water use. The "tools" for our understanding of such changes, however, has significantly improved since 1986. Other than the mention of using remote sensing (without details provided), the incorporation of new technologies and analysis techniques, as described in the literature, was not provided as strong justification.</p> <p>An attempt at a conceptual model was provided (Figures 1 and 2; without any figure captions), but it did not provide enough detail or depth to link with and support the study's objective. It's really not a model at all.</p> <p>The study is not justified by scientific means as presented in this proposal. It is essentially a \$1.9 million update of an internal bulletin, the contents of which will likely not reach the broader scientific audience.</p>
Rating	

Technical Review #1

	poor
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Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	<p>The approach, although systematic, is not hypothesis-drive, and therefore suffers from the complete lack of a detailed methodology that would systematically test each hypothesis and drive the research further. The approach is essentially an expensive literature review, and this will not generate any novel information. Having a location-driven bibliography available to decision makers would be helpful to them, but is this worth \$2 million?</p> <p>Redirecting efforts into first quantifying the post-1986 changes, then understanding the processes driving these changes, and then taking this understanding to forecast potential future changes would make this a proposal, with items 1 and 2 driven by well-formulated hypotheses. There is no science in the proposal in its current form- it will be a \$2 million Bulletin.</p>
Rating	poor

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	<p>The approach was not fully documented. There was too much vagueness throughout the proposal, and therefore one questions the technical feasibility of the</p>
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Technical Review #1

	<p>approach when it appears that the details were not fully thought out and developed.</p> <p>I believe an updated Bulletin will be the end product of this work, and is within reach of the authors, but I don't see what the value of it is. I don't see any novel techniques being used or developed, or the ability of this product to be used to aid in future water resource planning and management.</p>
Rating	poor

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	N/A
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The product is a 2-million-dollar Bulletin that will likely never be of value to the broader scientific community because there are no plans or discussion about graduate or undergraduate training, conference presentations, or production of manuscripts.
Rating	poor

Additional Comments

Comments	While I value the basic need to revisit and better understand what the authors propose, their proposal
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Technical Review #1

	<p>has major deficiencies, namely:</p> <ul style="list-style-type: none">- lack of novel techniques, analysis, or methods- no hypotheses- poorly developed and vague methods- no educational component- poor publication track record (16 staff, 4 PhDs, 3 pubs., all by one, in one journal, one "in prin" since 2002?)- structural problems (none of the refs. Cited in text!, no figure caption...)- incredibly large budget (nearly \$2 million!)
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Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	There is 16 staff listed on this proposal. Of those 16, 4 have Ph.D, 2 have Masters Degrees. They all have a great deal of practical experience, and the necessary available infrastructure to accomplish the project, as stated. The research productivity, as assessed by the publication record, is very low: three peer-reviewed publications, all in one journal, and only 2 as first-author.
Rating	fair

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget is outrageous. All of the requested funds are for salaries.
Rating	poor

Overall

Provide a brief explanation of your summary rating.

Comments	
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#0283: Quantitative Estimates of Evapotranspiration in California

Technical Review #1

	<p>My summary rating for each category is summarized in the table below. I calculated my overall rating as: $3+1+1+1+1+2+1 = 10/35$, equivalent to 1.43/5.00 (based on "Excellent" = 5... "Poor" = 1), which rounds to a "Poor" on your scale.</p> <p>Category Rating Summary Comment Goals G The idea is good, but not a single hypothesis developed. Not a science-driven proposal, just the update of previous Bulletin. Justification P Argued that big changes have occurred, but these changes were never described. Approach P Not hypothesis-driven research at all. Methods weak and vague. Feasibility P Much too vague, but feasible if an weak product is desired. Monitoring N/A N/A Products P A 2-million dollar Bulletin with no broader impacts. Capabilities F Almost no publication record of 16 PIs. Budget P Outrageous; nearly \$2 million.</p>
Rating	poor

Technical Review #2

proposal title: Quantitative Estimates of Evapotranspiration in California

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	<p>The goal of this project is to provide better estimates of evapotranspiration from agricultural and managed wetlands. The rational being that if better estimates of ET can be provided California can better manage its water resources. Fundamentally this proposal is a request from the California Department of Water Resources for updating Update and publish vegetation water use in the state of californial. detailed evaluation of various methods of et calculation and propose some standardization</p> <p>Proposal is by the department of water resources. Sounds like they want to update their last bulletin on ET (DWR Crop Water Use in California, last published in 1986). The proposed work would also include managed wetlands.</p> <p>The goal is clearly stated and consistent. In terms of hypotheses--no hypothesis is presented--which reinforces my impression that the work proposed here is not really about science.</p> <p>I would not rate this work as timely and important. The main outcome would be revised estimates of ET for different crops for a</p>
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Technical Review #2

	range of climate conditions. No new methodologies are being used or tested. Current state of the art in predicting agricultural ET is that of real time estimates based on current weather conditions.
Rating	fair

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	There is no conceptual model presented in this proposal other than that of the need to update estimates of agricultural ET. It is argued that new technologies have become available in the last 20 years that will be incorporated but nowhere in the proposal was it elaborated as to specifically what these new technologies are. The proposals suggest that the product will allow better estimates of groundwater recharge and will provide better input for hydrological models.
Rating	fair

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	This project appears to be basically a review and synthesis of information related to evapotranspiration from California agriculture. A review of methods for calculating ET, evaluation of input / output
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Technical Review #2

	<p>processing of several models (not clear what this would entail), an evaluation of pan evaporation data. From this information a product will be produced which is an updated estimates of ET. Results will be provided in a report, and electronically.</p> <p>What is unclear is how these new results will be produced. Overall, this is not particularly innovative and will not result in an appreciably higher understanding of agricultural et.</p>
Rating	fair

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	The project is feasible and the probability of success is high. The scope of the project seems consistent with the objectives and within the grasp of the authors. But there is nothing very imaginative or innovative.
Rating	good

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	No monitoring is proposed here.
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Technical Review #2

Comments	This activity would produce an updated "Crop Water Use in California". A product like this would likely be widely used. It just strikes me that an activity like this should be part of the normal activities of the DWR and not funded as a science project.
Rating	good

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	There are a great many people listed on this project. Most are trained at the bachelors or masters level. A few Ph.D. scientists have been listed but their level of involvement is not clear. Taken as a whole--the scientific creditials of this team are not very impressive. That said--I am sure they are fully capable of accomplishing the task laid out, if it is decided that this is important for the State of California. It may be an important tast but it is not cutting edge science.
Rating	fair

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	A budget of almost 2 Million dollars seems very high to me. I would venture to guess that most of what is proposed here could be accomplished by funding a couple of Ph.D. dissertations.
Rating	

Technical Review #2

	fair
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Overall

Provide a brief explanation of your summary rating.

Comments	As noted elsewhere--this is not an innovative proposal and sounds like something that if important should be funded as part of the normal activities of the DWR. With the level of funding requested more innovative approaches to estimating agricultural ET could be developed.
Rating	fair

Technical Review #3

proposal title: Quantitative Estimates of Evapotranspiration in California

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	<p>The proposal offers a concise summary of the goals and the purpose of the study, but lacks any fundamental justification for the work. The subject matter appears important and relevant to a myriad of interests related to water use in California, but the proposal does not offer any explanation of the current state of knowledge. For example, while the proposal endeavors to develop updated, more accurate vegetative water use information, there is no statement, summary, or analysis of the shortcomings of existing information. No background information is provided that demonstrates, empirically or qualitatively, the extent or magnitude of the discrepancies between current modeled estimates and actual vegetative water use. Moreover, there is no explanation, discussion, or analysis of the potential scale of the economic or environmental impacts of purported inaccuracies using current methods. In short, the proposal provides no justification relative to existing knowledge or knowledge gaps, provides no conceptual model for the studies, nor states a hypothesis the program proposes to test.</p>
Rating	poor

Technical Review #3

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	The proposal does not justify its project based on existing knowledge. The existing state of knowledge is not described, gaps are not identified, nor are suspected inaccuracies evaluated or analyzed. No conceptual model is provided that depicts the proposal's methods, metrics, or mileposts to justify the work. While a generic overview of applications of water use information is provided in Figure 1, and general water balance model is provided in Figure 2, the proposal lacks a basic conceptual model, in descriptive or illustrative form, that describes or justifies their approach or the need for the project. I find this to be a critical gap in the proposal because the proposed work would affect so many vested interests, it seems imperative to present a clear quantitative and/or qualitative justification based on the inaccuracies in existing methods, describe key changes in the system (e.g., irrigation technologies), and clearly state the key hypotheses to be tested.
Rating	poor

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The proposal does not offer sufficient
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Technical Review #3

	<p>information on the approach to determine if the project can meet its goals or objectives. No references are made to building on existing empirical data to address suspected empirical inaccuracies. Moreover, there is no information provided on where inaccuracies exist within existing models.</p> <p>While the project, by its subject matter, would have potential to add to our knowledge base and provide information of direct relevance to decision-makers and water users, there is no clear description of the methods the project will use to reach its conclusions. For example, the proposal states, "input, output, and processing components of ... several ... models will be compared and differences ... documented. This will provide an opportunity to select the best methodology among the models and apply it consistently in all of the models." Unfortunately, the proposal offers no description of the system relationships illustrated in different models, the metrics used to document differences, or how the differences will be evaluated. In several places the proposal states that information will be collected, but it does not provide any explicit data categories. One of the key reasons given for the work is that irrigation technology has evolved. No where does the proposal describe these changes, the extent or magnitude of their use, or how they are expected to affect current methods for calculating water use. The proposal reads like an overly simplistic positivistic approach with unstated assumptions that need to be made explicit and for challenge and testing.</p>
Rating	poor

Technical Review #3

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	<p>Because the approach is lacking critical details on the underlying assumptions, methods, and data to be used in the study, it is difficult to have confidence in the technical feasibility of the study. The project is not fully documented. In addition, the project proposes to add several apparently useful but tangential components that by themselves would be ambitious undertakings, such as some of the web based databases.</p> <p>It is not clear, for example, why the project has not proposed a web based self reporting form for much of their data collection efforts. This would serve several purposes. Foremost, it would force the project to make explicit their data categories and empirical data needs. It would also distribute the workload and directly facilitate the development of a database. Integrating such a web based such application into the overall research program would also demonstrate a level of organization and system-level thinking that would be required to successfully complete such an ambitious project.</p>
Rating	poor

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	<p>The proposal lacks key performance measures and does not provide basic quality control or quality assurance components. No information is provided on how existing models will be compared, or even what metrics will be used. There is no explicit provision for internal or</p>
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Technical Review #3

	<p>external technical or scientific peer review of reports, or where reports will be published (only that "reports will be published and posted on-line").</p> <p>Importantly, the proposal provides for no empirical field testing of its revised modeling. This appears to be a critical gap in the project because one of the main justifications presented for the work is new irrigation technologies.</p>
Rating	poor

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	<p>In general the products of the project appear to be directly useful and relevant to researchers and decision-makers. The proposal, however, does not lend confidence to the quality or utility of the products. For example, the first product is an annotated bibliography. The proposal itself, however, lacks proper citations and has a very inconsistent bibliographic format with numerous typographical errors. Since a key component of the program is to manage and evaluate data from multiple sources, the proposal itself fails to demonstrate a basic ability to complete this task.</p> <p>The proposed contents of the reports are too vague to gauge its value. One product consists of "Spreadsheets and digital maps of average available waterholding capacities for 20 crop categories for each Detailed Analysis Unit area and for zones of similar evaporative demand." The proposal does not describe the proposed contents of the spreadsheets (e.g., data categories), define the crop categories or a "Detailed Analysis Unit," or describe how geographic or content categories would be grouped or split.</p>
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Technical Review #3

	Lacking a proposed design for the databases, there is no way to evaluate the project's ability to create, manage, and disseminate information effectively. The fact that a design, model, or mock-up of the database was not presented, suggests that it has not been developed. Categories, units, data ranges, etc. need to be explicitly developed prior to building an effective and useful database. Consequently, the project does not appear ready to deliver this product in a timely, effective, and useful manner.
Rating	poor

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The project team, based on the resumes provided, appears to have the experience and diversity of technical expertise to implement the project. The only apparent gap in the staff's areas of expertise (remote sensing and web based database management) are outsourced to other providers, such as UC Davis. The scope of those outsourced tasks, however, is vague and poorly defined in the proposal. Consequently, it is unclear whether the team has the technical, organization, and infrastructural support required to accomplish the project.
Rating	good

Technical Review #3

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	<p>The budget is difficult to evaluate because the proposal does not provide the clarity and detail required to evaluate how the team members plan to spend their time on the project. The rate of \$100/hour for 455 hours to create an annotated bibliography seems high for this task that while fundamental, is probably more efficiently completed by more junior staff at lower hourly rates. While the average rate of \$88.5/hour is very reasonable for senior, experienced scientists, without additional information on how the task would be accomplished it is difficult to justify 1,668 hours and \$147,601 on "Reviewing methods of estimating ETo and Etc." Similarly, the proposal requests paying high hourly rates and \$178,950 to "Collect crop water use and water management information." This task would seem to be a simple administrative task completed by junior staff with low hourly rates once a data base format is established. The proposal requests to pay for a 1.5 years of a senior-level scientist time to review crop coefficients, \$137,291 for nearly a year to refine the delineation of evaporative zones, and 2.34 years to determine crop water use values. Unfortunately, the proposal provides no explanation for how this work would be done, nor a justification for the length of time required to accomplish such tasks.</p>
Rating	fair

Overall

Provide a brief explanation of your summary rating.

Comments	<p>The proposed project, on the surface, appears to be a valuable and useful undertaking for all water management interests in California. But such a</p>
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Technical Review #3

	contentious, high-stakes and high profile subject requires a more detailed, explicit, and transparent process and methodology to justify funding. It would need an integrated quality assurance and quality control plan with independent scientific peer review of key deliverables to ensure that the conclusions and products are received with confidence and legitimacy. While the assembled team appears to have adequate experience and expertise, the overall presentation and proposed implementation of the project is lacking in many critical areas to justify funding.
Rating	poor

